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Discussion of
"EQUATION OF THE FREE-FALLING NAPPE"

by Fred W. Blaisdell
(Proc. Paper 482)

FRED W. BLAISDELL,[1] M. ASCE.—Professor Rand is, of course, correct when he states that the equations presented in the paper apply only while the nappe is free-falling. They do not apply while the nappe is supported by the water cushion in back of the nappe or after the nappe plunges into a pool. Although gravity forces may accelerate the nappe when the nappe is supported by the water cushion in back of it, gravity can have no effect on the nappe shape after it plunges into a tailwater pool. The writer has developed an equation[2] for the form of the upper nappe trajectory below the tailwater level by assuming that the nappe undergoes no further vertical acceleration after it passes through the surface of the tailwater. In other words, the slope of the nappe below the tailwater surface is the same as its slope when it enters the tailwater. This discussion is an extension of Professor Rand's remarks to a more severe and probably more usual case.

The distance from the weir at which the tangent to the non-submerged upper nappe will become parallel to the apron is useful as a reference point for the location of energy-dissipating baffles, as stated by Professor Rand. Mr. Donnelly and the writer have found that the average distance at which the free and submerged nappes strike the apron is also a good reference point for the location of baffles and sills.[2] The actual point to use will probably depend on whether there is no submergence of the nappe, as in Professor Rand's case, or whether the nappe is submerged, as in the case of the straight drop spillway stilling basin.

The information presented by Mr. Harrold is most interesting and is a valuable addition to the paper. The agreement between the author's equation, which was developed from data obtained from numerous sources, and the Corps of Engineers equation, developed from the Bureau of Reclamation data alone, is shown by Mr. Harrold to be excellent. This should engender confidence in both equations.

The writer wishes to thank Professor Rand and Mr. Harrold for taking time to make their interesting and constructive comments.

1. Project Supervisor, U.S. Dept. of Agriculture, St. Anthony Falls Hydr. Lab., Minneapolis, Minn.
2. Straight Drop Spillway Stilling Basin, by Charles A. Donnelly and Fred W. Blaisdell, University of Minnesota, St. Anthony Falls Hydraulic Laboratory, Technical Paper No. 15-B, November, 1954.

Discussion of

"DIVERSION FLOW THROUGH BUFORD DAM CONDUITS"

by Francis F. Escoffier
(Proc. Paper 535)

FRANCIS F. ESCOFFIER,* A.M. ASCE.—The writer is indebted to Mr. Tults for an interesting and informative discussion.

Under (a) Mr. Tults questions the accuracy of the solution for critical depth obtained by using the F-curves as compared with that obtained by using his eq's. 1, 2 and 3. Actually the two solutions are mathematically equivalent and the accuracy obtained when the F curves are used depends on the scale to which the curves are plotted and on the care with which the construction is carried out. The Coriolis coefficient C_v is readily introduced into the graphical method by redefining the function F as follows:

$$F = \frac{C_V}{2gA^2}$$

This step has already been taken elsewhere. (See footnotes 1 and 2 in the writer's paper.) The writer did not feel that this refinement was needed in his paper and therefore did not use it.

In channels where the effect of friction is negligible a considerable advantage arises from the use of the F curves if the location of the cross section in which flow at critical depth occurs is not known in advance. In this case F curves representing a number of cross sections are drawn and a line having a slope equal to $-Q^2$ is drawn in the lowest possible position which permits contact with all of the curves. Such a line will then be found to be tangent to the curve for the cross section where the water flows at critical depth.

It is true, as Mr. Tults states under (b), that curvature of the filaments has an adverse effect on the accuracy of the graphical method and that a flow net can be used to improve that accuracy. However, the drawing of flow nets is practical only when the flow pattern is two-dimensional. In the Buford penstocks, where some effect due to the curvature of filaments might be expected, the side walls diverge and the flow is not two-dimensional.

Mr. Tults has performed a valuable service in bringing together the information presented under (c). There is no reason, however, why this information cannot be used in connection with the graphical method presented in the writer's paper. The value of the Manning roughness number n used in reading off transition levels from the curve in Fig. 5 need not be the same as the value used in eq. 2 of the writer's paper to determine the outlet-control point I. If the Schoklitsch correction represented by Mr. Tult's eq. 4 is to

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be used, it will be necessary to modify the curve given in Fig. 5 of the writer's paper by multiplying the abscissa by the ratio

$$(1 + \alpha \frac{b}{p})^{\frac{4}{3}}.$$

Discussion of
"THE PRESENT STATUS OF RESEARCH ON SEDIMENT TRANSPORT"

by Ning Chien
(Proc. Paper 565)

NING CHIEN,* A.M. ASCE.—Owing to the large number of papers published in the field of sediment engineering, it is almost impossible to cover every phase of the research which has been carried out by various persons and agencies. It was the writer's hope that some of the important research programs, which have not received enough attention in the writer's summary, will be reported in more detail by those who are familiar with these programs. In this respect the discussions contributed by Messrs. Lara, Rupani, Stall, and Liu are especially constructive and valuable. Also Mr. Blench's suggestions of using field observations are highly supported. Empirical laws, however, which are derived from field observations in certain types of channels should not be applied to different types of channels without previous checks in such channels.

At low or moderate flows when the bed is covered by sand bars, the motion of bed-load can be determined either by measuring the transport rate of individual particles in the bed layer, or by measuring the collective motion of sand bars. However, a bed-load formula in terms of the collective motion of sand bars does not necessarily mean to be better than the existing ones which are derived on the basis of individual particles' motion, as suggested by Mr. Liu. Not only the basic mechanics which initiate the sand bar formation are not well understood as yet, but the link between the motion of bed-load and the distribution of suspended sediment will be lost if the former is expressed in terms of the collective motion of bed particles. It is the writer's opinion that enough bed-load formulas have already been proposed, and any new formulas developed on the basis of the same data probably will not differ materially from the existing ones, even if they may assume different mathematical forms. It is much more important to extend the range of application of the existing theories and to determine their limitations. Mr. Lara has mentioned the difficulties in applying the prevailing methods to determine the loads carried by cobble-bottom channels. An even more important step is the extension of present theories to streams which drain watersheds supplying silty sediment at high rates. This is especially true in arid areas with only rare but heavy precipitations and with large deposits of unconsolidated silt ready for erosion. The extremely high concentration of silty sediment at all significant flows makes the division of flow into a liquid and into a solid phase illogical, as pointed out by Messrs. Rupani and Stall. Liquid and solid actually blend into a liquid of entirely different characteristics, such as density and viscosity with all possible stages of transition

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from water to a mud flow. Mr. Lara also called for research on developing a method for computing the degradation below dams. Attention is called to an excellent work by Harrison,[A] which has not been reported by the writer, as it falls into the field of sediment erosion. In the same general field the work by Laursen[B] also warrants recommendation. Information is equally meager in the last phase of the sedimentation process—sediment deposition, with the exception of deposition in reservoirs.

Mr. Lara has reported the work carried out by field engineers in modifying the Einstein method to reduce the necessary field work. In spite of the fact that such attempts have been proved beneficial to field workers and should definitely be encouraged, nevertheless the writer wishes to express a few words of caution. The Einstein method covered a wide range of conditions, as reported in the writer's paper. Changes in any one phase of the whole description must be thoroughly checked against all these conditions. Had this not been done, such modifications may give better results in a certain range of conditions, but may lead to gross error in other ranges. Furthermore, every step of development which leads to the Einstein bed-load function of its present form has its physical significance. For instance, the use of Eq. (2) is based on the linearity of friction in open channels, which allows the linking together of grain roughness K_s with its corresponding hydraulic radius R_b'. The replacement of R_b' in the logarithmic term in Eq. (2) by the total depth d may simplify the computation procedures, but, in the meantime, also divorces the equation from its physical background. Although practically speaking such a step has little consequence, as any moderate change in a variable affects the logarithm of the variable even to a lesser extent.

Some of the results as quoted by the discussers appear to be in error. The one-half power in Eq. (39) has been verified according to the investigations of Leopold and Maddock.[75] The power 0.26 as reported by Leopold and Maddock and as quoted by Rupani and Stall applies only to the variation in a particular cross section of a river. The Einstein curve prepared by Hanson and cited by Lara was based on the old Φ-Ψ relationship,[59] and should be reconstructed according to the new Einstein bed-load function.[3] This will eliminate the part which gives almost constant sediment load at a range of SR values. In reality this part of the curve has not much physical significance, as rivers with bed material size of 0.125 mm. seldom, if ever, flow at a product of SR higher than 0.005.

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C. "Author's Closure—Graphic Design of Alluvial Channels," by Ning Chien, Proc. Separate No. 611, 1955.

H. A. EINSTEIN,** M. ASCE.—Before his departure from this country, the author authorized the writer to answer in his place any further comments referring to his paper.

**Prof., Mech. Eng., Univ. of California, Berkeley, Calif.

Mr. Blench makes it an important point of his discussion that the superiority of his "simple and adequate" formulae is a priori proved by the fact that they are derived from field measurements in full-sized canals rather than from flume experiments with "trifling" discharges. The writer would like to take issue with this statement. Every empirical equation is adequate, at least in the eyes of the engineer, if the constants of that equation are known and can be predicted for every case which wants to be calculated. In following the recent papers and publications on the development of Mr. Blench's "simple and adequate" equations, the writer noticed that even the proponents of that general approach do not at all agree on the choice of these constants and that not even general agreement exists on the parameters upon which they depend. The American engineer who lacks the necessary years of experience with these formulae thus stands a grave chance of choosing wrong constants and of emerging with a wrong result, despite the correct use of the formulae. This is naturally exactly the same with any other equation such as the Manning formula, and the best results will always be obtained if the engineer uses that formula for which he knows the constants best and with which he has the most experience.

If Mr. Blench maintains that bed-load rates can not be measured in nature, he must not be familiar with the rather large list of such measurements which have been made in Austria, Switzerland, Holland and in the U.S.A. What he rather wants to state is that the measurements in the natural canals with the higher than trifling discharges on which his formulae are based, did not include the determination of bed-load rates. But even a quick check into the Indian canal data reveals that all data were taken in a very narrow range of bed-load conditions. Even if the bed-load rates had been measured, no interesting results could have been revealed. The writer is rather doubtful if the derived formulae can be assumed to still hold under changed bed-load conditions such as exist in many American rivers.

It may interest Mr. Blench to know that the above mentioned river measurements of bed-load transport also provide a complete hydraulic measurement of the flows and have permitted the proof of applicability to a wide variety of rivers of the formulae which the writer and others have derived from flume experiments with trifling discharges.

With respect to the introduction of the bed-load rates as a weight concentration only, the writer does not believe he is able to follow Mr. Blench's arguments. The writer has seen rivers carrying 1% of gravel and others carrying 1% of silt or fine sand. He believes he remembers that the effect of the two loads on the streams was distinctly different and can thus not be expressed by the concentration only.

As a last and concluding remark, the writer would like to agree heartily with Mr. Blench's idea that the sediment problem can be solved only by a combined attack in the laboratory and in the field. It is a fallacy, however, to think that field measurement is identical with regime theory. The writer is actually glad to have spent a number of years in the field studying natural stream behavior, and it is this prototype study which has guided him in the organization of the laboratory work and its analysis, and which has forced him to view the general applicability of the regime theory with suspicion.

Discussion of "SIMILARITY OF DISTORTED RIVER MODELS WITH MOVABLE BED"

by H. A. Einstein and Ning Chien
(Proc. Paper 566)

H. A. EINSTEIN,[1] M. ASCE.—First, a word of disappointment, as no comments have been voiced by engineers from the U.S.A. who are practically involved in the construction and interpretation of models in our large laboratories. Are they not interested in this problem?

Professor Thomas Blench gave his opinion of the proposed formulas from the viewpoint of regime theory, repeating in part the more detailed procedure of deriving similarity laws from those equations. Most engineers may fast be getting tired of this steady fight between the two methods by which sediment-carrying channels are being calculated, and deserve an explanation of the basic differences between the two methods. The regime theory, as represented by Blench on the one hand, and the American-European method, represented by the writer's "bed-load function" on the other.

The regime theory was essentially developed in India by various engineers from field measurements in artificial canals of rather uniform character, but widely varying size. Three equations were developed which can be brought into a form saying that any one of the three variables: average depth, average velocity, and channel width, are unique functions of the discharge. These functions were obtained by plotting canal data on log-log paper and by drawing the best fitting straight lines through the points. This was easily possible due to the inherent regularity of the canal data which were all either running at design discharge or were empty. A stage-discharge relationship as we know it for rivers does not exist in these canals.

The resulting relationships thus had the form

$$\text{variable} = \text{constant} \times Q^{u}$$

where the variable is any one of the three variables just enumerated, u is supposedly a universal constant in each equation, and the constant is supposed to be an unspecified function of sediment grade, sediment load and maybe some other factors. What looks very nice for the Indian canal data (from which many were excluded as not being in equilibrium!) does not look so good if applied to American natural rivers. Fig. A gives as an example the channel width b in terms of the discharge Q for the river sections which the writer used previously in his ASCE paper on "River Channel Roughness"(1) compared with the predictions of the regime theory as published in Publ. 20 of the Indian Control Board of Irrigation.(2) It may be seen that not only is the constant in most cases wrong by a factor of 3, but also the slopes of the various river curves are different among themselves and definitely different from the regime prediction. It even appears questionable if a straight line

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may always be applied to describe river behavior. To test that, the average velocity in the same stream sections was plotted against the average depth in Fig. B., and the reader may draw his own conclusions about the ability of parallel straight lines to describe alluvial river conditions. Here the deviations from the straight line are so pronounced that the writer's proposal(1) appears to be a distinct progress. While the writer used basically the Manning equation, Mr. Blench uses the regime line. It is hardly possible on the basis of this graph to understand the great superiority of the regime formula claimed by Mr. Blench, particularly if one takes into account that the constant of the Manning formula can be predicted, that of the regime formula must be guessed at, and that the correction for bar resistance is applied to the Manning formula while the regime formula does not need correction by "dubious" means.

The bed-load function, on the other hand, attempts to give the best possible description of the sediment and flow picture in model and in prototype, and to compare the two by means of model laws. If proponents of the regime theory think that they can describe the alluvial river flow by hiding the sediment transport in some mysterious constants (bed factors), they are badly mistaken. Sediment transport is one of the most important parameters of the problem and needs to be studied as such.

After this rather long, but necessary introduction, it is possible to discuss the similarity itself. Similarity is most easily established for power functions of the type used in the regime theory, but gives good results only if the flows actually follow those curves. The method described in the paper where relationships are approximated in both scales by power functions of equal exponent and the ratio of the constants thus determined, is an enlightened use of something similar to regime theory. The difference from Mr. Blench's proposal is that one takes here a known and calculated risk and that the deviation from the true similarity can be estimated.

The even more important method of applying similarity is through the means of general relationships between dimensionless variables. Such relationships are usually not just power relations. In that case the individual dimensionless parameters entering the equations must be made equal in model and prototype. This method is used in this paper; for instance, in the case of Φ, ψ and others, while regime theory does not give the necessary relationships explicitly, to derive these similarity conditions. They hide in the bed-factors as explained previously.

In closing, it may be repeated that it is quite deplorable that more men with the necessary model-prototype experience did not enter the discussion. Maybe the next years will see some efforts of proving or disproving the contention that model scales of such distorted models of alluvial rivers with movable model bed can be predicted and designed without much trial and error.

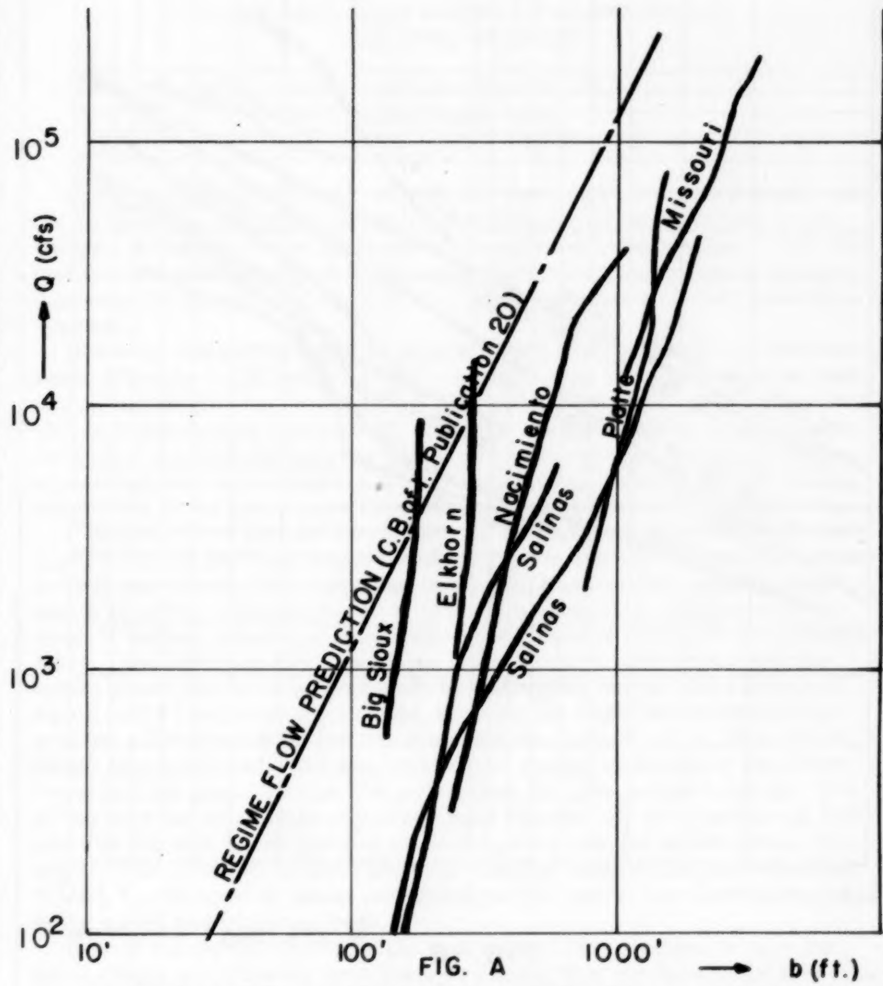

FIG. A

COMPARISON OF THE MEASURED WIDTH OF VARIOUS AMERICAN RIVERS IN TERMS OF DISCHARGE WITH THE PREDICTION OF THE REGIME THEORY.

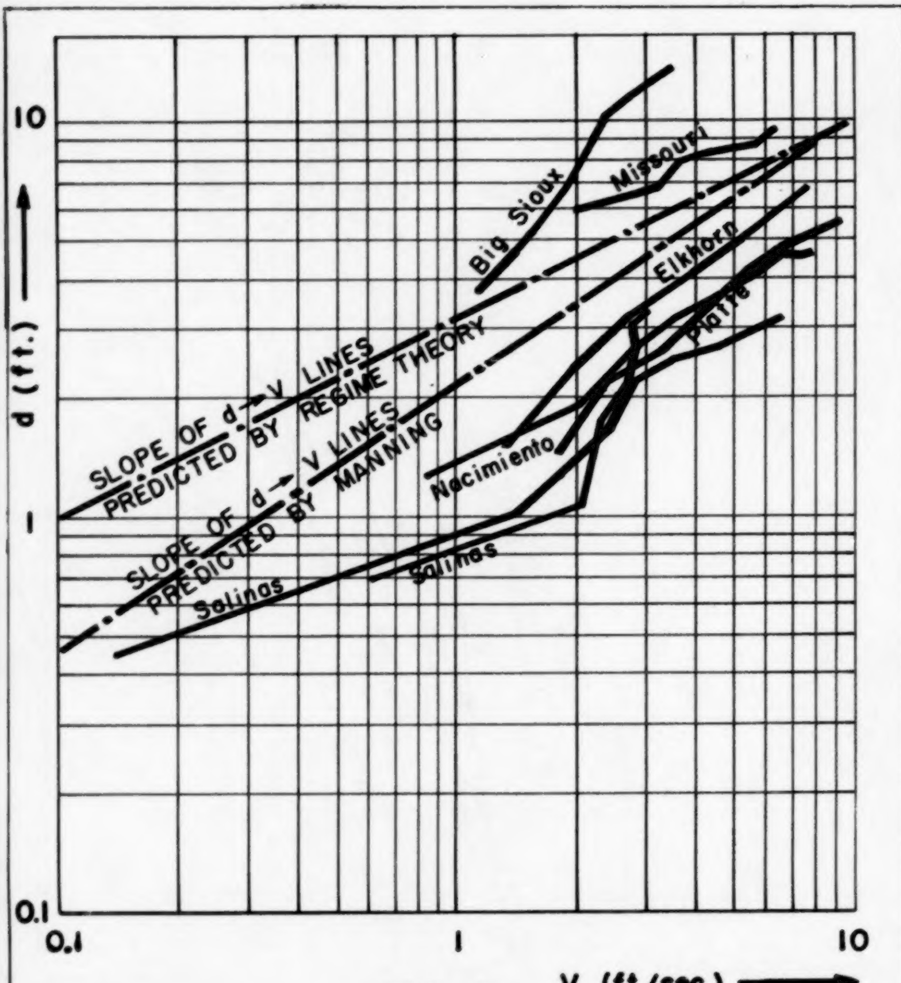

FIG. B

COMPARISON OF THE MEASURED RELATIONSHIP BETWEEN DEPTH AND VELOCITY WITH THE PREDICTION OF THE REGIME THEORY.

Discussion of
"SCALE RELATIONS AMONG SAND-BED RIVERS
INCLUDING MODELS"

by Thomas Blench
(Proc. Paper 667)

HANS ALBERT EINSTEIN,[1] M. ASCE.—The writer is highly pleased with the fact that the author has based his entire paper on the main idea of developing similarity rules on the basis of empirical relationships.[1,2,3] The fact that the author does not explicitly refer to these papers which propose this same method is highly appreciated as an expression of their general acceptance.

However, beyond this most general and basic agreement, the writer has some difficulty in following the author's arguments; for instance, if he states that Froude's law "has nothing to do with hydraulics," one must wonder what kind of hydraulics he has in mind. That the similarity rules as proposed by the author are derived from the water-sediment regime-complex is not unexpected to the regular reader of the Proceedings. That the results are as unsatisfactory as shown does not speak very highly for the regime theory.

It is quite clear that the conclusions of the paper stand and fall with the applicability of the basic equations of the regime theory. The author's equation (4) was selected for a test and in Fig. (A) this equation, with the constants as given by Gerald Lacey in 1939,[4] is compared with observations taken in various American rivers which were used in paper,[5] Proceedings, ASCE. One observes that none of the seven characteristic river reaches comes even close to being described by the regime curve. All curves are significantly steeper than predicted, and also the width values themselves are, for all rivers except the Big Sioux and the Elkhorn, about three times larger than predicted. The constant for the regime relationship was taken from the 1939 paper because the author does not give any such values. The writer was not able to find any newer such figures. No such numerical data could be found at all for channel sizes comparable to the model river. The author is thus invited to quote any such data that support his conviction that F_b and F_s are equal in model and prototype if identical bed material is used in the model and prototype beds.

Even if the author visualizes the constants of his equations to vary for some rivers considerably from Lacey's values, it is not understood how laws with much different powers can describe the river behavior satisfactorily. This deviation becomes even more significant if the newest edition of equation is used which Inglis[6] (1948) gives as

$$b = a_1 \frac{Q^{1/2}}{g^{1/3}\nu^{1/2}} \left(\frac{X \cdot V_s}{m}\right)^{1/4}$$

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Herein are b the channel width

a_1 a constant

Q the discharge

g the acceleration due to gravity

ν the kinematic viscosity

X the ratio of bed load rate to discharge

V_s the settling velocity of the sediment

m the diameter of the bed sediment

For any one reach a , g, ν, V_s and m can be taken to be constant and X is well known to increase with increasing Q, so that the resulting width b grows even faster with Q than in the original equation (4). In Fig. (A) Inglis' relationship is demonstrated by a line even flatter than Lacey's curve and, therefore, fits the river curves even less.

The question arises now as to why these laws which are based on sound observation deviate so basically from our own river observations. There may be various reasons, of which the following two are cited here:

1) In the analyses of the data leading to the various formulas of the regime theory, the assumption was made that all relationships can be expressed by pure power functions. Elimination of all other functions represents a severe handicap of the method.

2) The basic data were artificial channels with almost constant discharge. These channels were built with predetermined cross sections and do not permit the observation of stage-discharge relationships, which are one of the most important characteristics of any natural river.

In order to demonstrate the shortcomings due to the first point, the data of Fig. (A) are used to demonstrate that also some of the other relationships can not be expressed by pure power functions. Fig. (B) represents in graphic form the relationship between water depth and flow velocity for various natural American rivers. It may be seen that most river reaches show a very characteristic deviation from a straight line. It is quite understandable that the followers of the regime theory disagreed heartily even with the idea of explaining these deviations, which was attempted by the writer in paper,[5] Proceedings, ASCE, since such an attempt conflicts with one of the basic principles of their theory. The writer, on the other hand, feels more convinced by river measurements than by the abstract principles of a theory.

From this we may conclude that the regime theory has its great shortcomings in the description of flow conditions in natural alluvial rivers. It is not known to the writer how much experience exists in the application of this theory to models, and if the same exponents and constants apply to both scales. Paper 667 is rather vague on this point. After this review the writer does not feel encouraged to apply regime principles to the design of models for the solution of problems in alluvial rivers.

Let us write down some of the similarity ratios which the author is not able to predict from the regime theory. The following are taken from page 4 of paper 667:

1) the sand size
2) the sediment discharge
3) the composition of the transport
4) the density of the model sediment,

to enumerate just four which other theories permit to be determined.[3] Even more interesting are the rules of similarity derived from the theory and proposed by the author:

1) Equal Froude number, despite the fact that Froude number "has nothing to do with hydraulics."
2) Use prototype sand in the model,

to mention only the two which are straight laws rather than rules of how to find model conditions empirically by trial and error. First the question may be asked how Meyer-Peter[1,2] should have built his Rhein model on this basis with a water depth of 1-2 inches using prototype sediment ranging from 1/2 to 12 inch diameter. However, if this case must be excluded as not a "sand stream," the author is invited to advise the U.S. Army Engineers at Vicksburg why they were unable to duplicate flow and sediment conditions of the Mississippi River in a model using river sand as model sediment, until they abandoned the use of Froude's law by introduction of a significant distortion.

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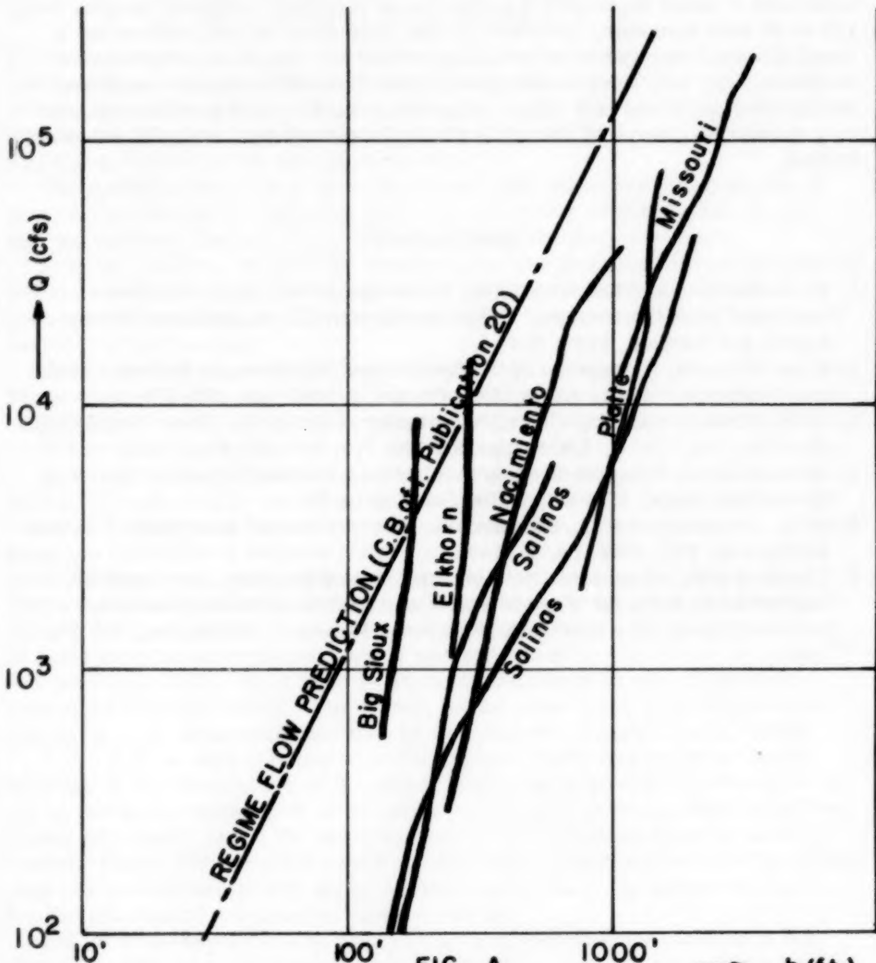

FIG. A

COMPARISON OF THE MEASURED WIDTH OF VARIOUS AMERICAN RIVERS IN TERMS OF DISCHARGE WITH THE PREDICTION OF THE REGIME THEORY.

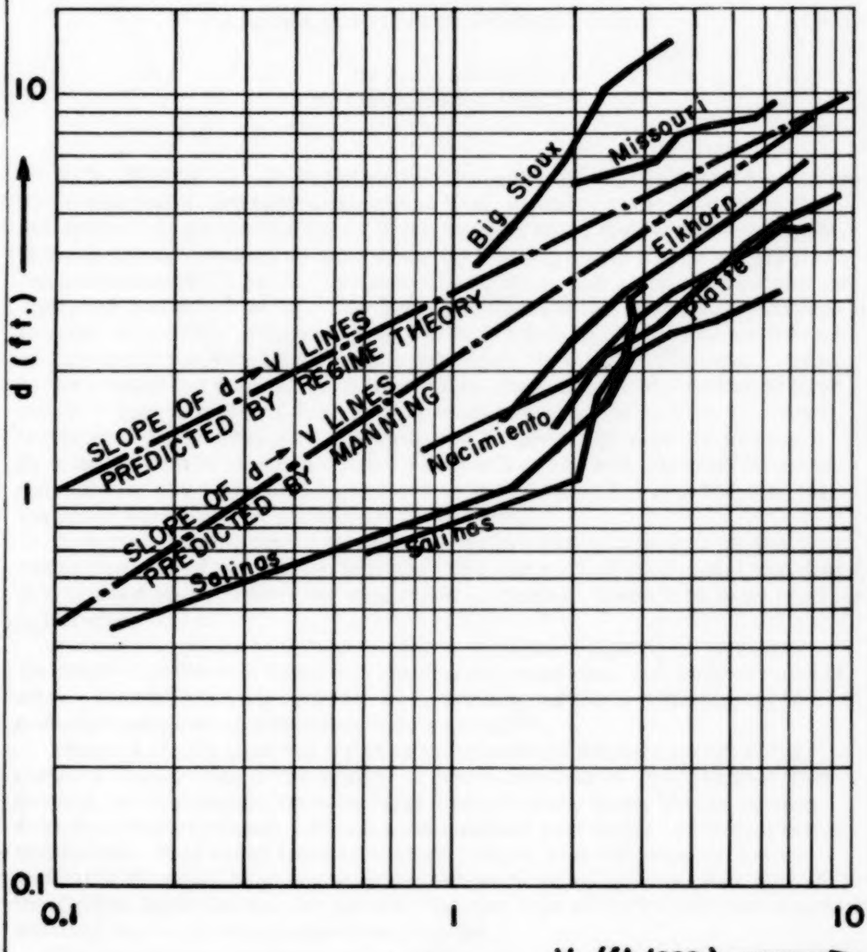

FIG. B

COMPARISON OF THE MEASURED RELATIONSHIP BETWEEN DEPTH AND VELOCITY WITH THE PREDICTION OF THE REGIME THEORY.

Discussion of
"A MORE SIMPLIFIED VENTURI TUBE"

by J. C. Stevens
(Proc. Paper 678)

R. B. DOWDELL.[1] —It is felt that a discussion of what is meant by accuracy of differential producers is appropriate. A recent paper(1) defines the "tolerance" of the coefficient as being equal to twice the standard deviation. Without getting involved in theoretical terminology, this means simply that the coefficient of 95 out of 100 tubes will be within the given tolerance of the specified coefficient value. For Herschel-type Venturi tubes this tolerance is given as ± 3/4%. This means that it is possible to predict the coefficient of Herschel-type Venturi tubes to within 3/4% in 95 out of 100 cases. Here is the crux of the matter. It is possible to place almost any partial obstruction in a pipe line; and if it is calibrated in place, it will operate as a perfectly good differential producer provided its geometry does not change. Granted there will be some shapes which will produce steadier differentials than others, and some with higher losses than others, the fact remains that the basic phenomena of Nature are repeatable.

However, it is expensive to calibrate differential producers. In some cases, the cost of calibration is higher than the cost of the Venturi tube itself. If it is possible to predict the coefficient accurately, then there is no need for calibration.

It should be pointed out that the above-mentioned figures on tolerance of Herschel-type Venturi tubes are not merely guesswork, but were arrived at after a careful analysis by Prof. A. L. Jorissen of Cornell University of over 200 independent laboratory calibrations.(2)

Figure 3 of this paper is a plot of the results of the calibration of the author's Venturi tube. The scatter of points amounts to 4%. Analysis(3) of several calibrations of Herschel-type Venturi tubes shows that in no case does this scatter exceed 0.3% once the constant part of the coefficient curve is reached. This could mean two things. First, that the laboratories in which the Herschel tubes were calibrated were more accurate than that of the Oregon State College, or second, that this type of device has less inherent stability than a Herschel-type Venturi tube.

The use of very low water differentials also brings up an interesting question. If it is so advantageous to use these small differentials, why not go to much larger throats in Herschel-type Venturi tubes and also use low differentials?

There are two reasons why this has not been done. First, it was found, after much experience, that the differentials produced with tubes having a ratio of throat diameter to line diameter greater than 0.75 were not as reliable as those with smaller diameter ratios; and the coefficients were not as easily predicted. It was also found that these tubes were much more

1. Fluid Mechanics Engr., Builders-Providence, Inc., Providence, R. I.

sensitive to upstream conditions, and fittings like elbows, increasers, and decreasers had a pronounced effect even when installed a considerable distance upstream from the tube. This same failing has occurred in other types of differential producers including orifice plates and nozzles.

The second reason for not using larger throats and lower differentials concerns the operation of the secondary instruments over a wide range. The author points out that a foot of water can be read with the same degree of accuracy as a foot of mercury. This may be true, but a foot of water will not actuate a float-driven secondary instrument with the same accuracy as a foot of mercury. This is because the forces acting on the float are in proportion to the density of the fluids. Suppose it takes 0.005" movement of mercury to overcome the lag in a given instrument. It will take a change of 13.57 times this distance or 0.068" of water to overcome this lag if the same instrument is driven by water. In the mercury driven instrument, this lag will cause an error of .02% in flow at maximum rate and 2% at 1/10 maximum rate if the maximum differential is 12" of mercury. If the maximum differential is 12" of water, however, the error in flow will be 1/4% at maximum rate and 25% at 1/10 maximum rate.

The author uses an example in Table 2 of a differential of 0.48 ft. of water for 6 cubic feet per second flow, compared with a 12" x 6" Herschel tube with a differential of 13.8 ft. H_2O at this same flow. If these tubes are to be compared, the useful measuring range must not be neglected. Ordinarily a Herschel Venturi tube together with a standard instrument is recommended for a 10 to 1 range. This means that the above tube and instrument combination should measure accurately down to 0.6 cfs. At this flow the Herschel Venturi will produce a differential of 0.138 ft. of water, but the author's tube will produce only 0.0048 ft. of water. To measure this within 2% requires an instrument sensitive to .0001 ft. of water. This writer knows of no commercial instrument which will respond this accurately. If the new recorder which the author mentions will perform accurately at these extremely small differentials, he is to be congratulated.

Over the years, the custom of presenting head loss figures as a percentage of differential has grown, and with good reason. The primary purpose of a Venturi tube, nozzle, orifice, or Dall tube is to produce a differential to drive some type of secondary instrument. The efficiency with which it performs this function should be measured by the energy which is lost in performing it. The percentage of differential lost gives us the inverse of this efficiency, and tells how well the differential producer is performing. The presentation in Table 2 does not give an indication of this efficiency. Starting with the knowledge that a 12" x 6" Herschel-type Venturi loses about 10% of its differential, the writer was able to calculate the percentage loss in the Stevens tube at 5 cfs and a differential of 2.56 ft. of water. The value determined was 25% of the differential and is 2-1/2 times the loss in a Herschel-type Venturi tube and almost 4 times the loss of a Dall Flow tube with a 0.5 diameter ratio.

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PROCEEDINGS PAPERS

The technical papers published in the past year are presented below. Technical-division sponsorship is indicated by an abbreviation at the end of each Paper Number, the symbols referring to: Air Transport (AT), City Planning (CP), Construction (CO), Engineering Mechanics (EM), Highway (HW), Hydraulics (HY), Irrigation and Drainage (IR), Power (PO), Sanitary Engineering (SA), Soil Mechanics and Foundations (SM), Structural (ST), Surveying and Mapping (SU), and Waterways (WW) divisions. For titles and order coupons, refer to the appropriate issue of "Civil Engineering" or write for a cumulative price list.

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c. Discussion of several papers, grouped by Divisions.